Kühn and Mazzeo Reply Van Enter, Külske and Maes (EKM) in their comment [1] to our letter [2] point out – correctly – that joint probability measures on the product of spin- and disorder-space in systems with quenched randomness are non–Gibbsian. They conclude from this – incorrectly in view of the connotations carried by such a characterization – that our approach which provides approximate descriptions of such systems in terms of a net of Gibbs measures suffers from being ill defined.

To wit, it has been shown [3] that the approximation scheme used in [2] provides a *variational family of increasing exact lower bounds* for the free energy of systems whith quenched randomness in terms of Gibbs measures on the joint spin- and disorder-space. These measures agree on an increasing set of moments with the measure characterizing the quenched disorder. Moreover, at each level of the approximation studied in [2,4] the variational problem is convex (also in the thermodynamic limit), hence its solution unique. In this sense, our approximation scheme is perfectly well defined, and it is legitimate to investigate its efficiency in analyzing unsolved problems in the physics of systems with quenched randomness.

The joint measure related to the $2D$ spin-diluted Ising model studied in [2,4] is Gibbsian in the high-temperature phase, while only weakly Gibbsian in the ferromagnetic low-temperature phases (for definitions see the literature cited in [1]). Given the representation of the approximate measures studied in [2,4], it is perfectly conceivable that they could approach the joint measure of the quenched system, if one were able to carry the approximative scheme to its end. While a formal proof of this statement may be difficult, it nonetheless indicates that differences between the limiting measure of our scheme and the weakly Gibbsian measure it attempts to approximate in the low temperature phases might be rather subtle.

Rather than formal convergence properties, which ultimately require clarification, our main issue from a pragmatic point of view is the *efficiency* of the approximations in describing physical properties. As is quite common, we are only able to work through the first few steps of the scheme, hence the pragmatic question: Is the physics we are thereby missing essential or not? As far as one can presently see, it is not. Our findings concerning critical phenomena in [2,4] are fully in line with currently available empirical evidence coming from other conventional approaches, as well as with known exact results. The fact that critical behavior is, nevertheless, still under debate for the $2D$ spin-diluted Ising model is related to the difficulty in disentangling the finite size signatures of the conflicting scenarios from the numerical data, irrespective of what kind of (efficient) approximation is being used to obtain them [4].

The physics we *are* likely to miss generally is that associated with Griffiths' singularities [5]. We have been able to prove this for the $1D$ system [3]: the simplest approximation in this case (system (a) of [2]) is exact in zero field $h = 0$, and it also very accurately describes critical behavior and scaling in the vicinity of the multicritical point $T = h = 0$, $\rho = 1$, but fails to exhibit Griffiths singularities. The absence of these very weak essential singularities, while certainly a drawback, does in the light of available evidence not appear to be a very serious one for the problem studied in [2,4].

The important message of EKM is that formal convergence issues of our approach have indeed not yet been dealt with in a satisfactory way. A number of such issues occurring in situations where non-Gibbsian measures do play a potentially worrying role have been studied before, mainly in the renormalization group (RG) context (see in particular [6], and other references in [1]).

However, our case is also different from the RG setting in essential aspects, the perhaps deepest one being that the disorder potentials used to express approximating measures are invariant under *local* spin-reversal, a feature which may well remove many of the pathology generating mechanisms occuring in the RG context.

In summary, there is clearly nothing automatic about the success of our approach, nor, however, about its potential failure due to some formal similarities with the RG situation. Independent evidence must in any new case be checked. We have done no less in [2,4], with clearly encouraging results.

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1 A. C. D. van Enter, C. Külske, and C. Maes, Comment on [2].
2 R. Kühn, Phys. Rev. Lett. **73**, 2268 (1994).
3 R. Kühn, Z. Phys. B**100**, 231 (1996); and unpublished.
4 G. Mazzeo and R. Kühn, Phys. Rev. E **60**, 3823 (1999).
5 R. B. Griffiths, Phys. Rev. Lett. **23**, 17 (1969); see also Ref. 8 of [1]
6 A. C. D. van Enter, R. Fernández, and A. D. Sokal, J. Stat. Phys. **72**, 879 (1993)